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PROBLEMS OF DESIGN IN
PARAPSYCHOLOGICAL EXPERIMENTS

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AN experiment in parapsychology may be more time-consuming and less informative than it should be through faults in design. In bad cases of faulty design, the loss of information may be so great that no valid conclusions can be drawn even though the rate of success was great enough to lead to well-founded conclusions if the experiment had been well designed. Problems of design include such matters as the apparatus to be used, its methods of use, the precautions to be taken against the influence of such factors as normal sensory cues or unwanted psi processes, the number of experiments per session, the number of experimental subjects, the number of experiments per subject, the arrangement of experiments of different types, and the statistical methods to be employed in working out the results. A preliminary necessity which determines the details of design is a clear formulation of the question or questions which the experiment is designed to answer.

Faulty experimental design is very often a result of the experimenter being unclear on one or all of these matters when the experiment is started. He becomes progressively less unclear as to what he is trying to do as he goes along, but not before much time has been wasted. When the experiment is finished he tries to extract an answer from its results, although it may well be that the results have not been obtained in a form to which any of the standard forms of statistical estimation is appropriate.

In contrast to such haphazard method of experimentation, the ideal is that all details of design should have been thought out and written down before the experiment is started. It is true that in any investigation the problems may only become clear as the work proceeds. Such preliminary clarification of problems and procedures should, however, take place in an initial stage of the investigation which may be called the 'exploratory' stage of experimenting. While engaged in exploratory work, one's methods may be as fluid as one likes. No conclusion will be

drawn from the exploratory experiment ; its purpose is to provide guidance in the design of the main experiment. When it is over, one should be in a position to write down the full details of the design of the main experiment, and subsequently to modify this design only in trivial details or not at all.

Problems of experimental design are largely common to all the biological sciences because in all of them one is likely to be dealing with causal sequences in which the cause A does not invariably produce the result B but only tends to be followed by the result B with a certain degree of likelihood. The psychical researcher may, therefore, study with profit the works of such writers as Sir Ronald Fisher whose *Statistical Methods for Research Workers* and *The Design of Experiments* are valuable guides to the varieties of methods by which experimental problems may be attacked. He may not find that all parts of these works are necessary to him, but he will profit by study of those parts that bear on his problems.

Three requirements of a well-designed experiment are : (1) rigidity, (2) fruitfulness, (3) economy.

The requirement of *rigidity* is that the experiment must really prove what it sets out to prove. Success in an experiment designed to test ESP, for example, must not be explicable in any other way ; by such normal sensory cues as the subject seeing the figure from the back of the card, by unconscious signals given by the experimenter, by the subject using fraudulent methods of finding the right responses, by errors in recording either of targets or responses, and so on. In critical experiments which are designed to test the reality of paranormal phenomena, it is also necessary to complete rigidity that adequate steps should be taken to make it impossible that the results should be due to fraud on the part of the experimenter.

An additional system of precautions is necessary to ensure that results are not due to some other paranormal process than the one that the experiment is intended to test. In precognition experiments, for example, it is necessary to ensure that success could not be due to the experimenter shuffling the cards into an approximation to the predicted position, or influencing a machine to do so by P K.

In P K experiments, on the other hand, successful results may be attributed to precognition if (as in many of the early experiments) the subject is allowed free choice of his target before throwing.

It may indeed be impossible to ensure that any experimental task intended to test one paranormal process is not performed by another, if the second paranormal process is assumed to work with 100% efficiency. Otherwise the ordinary safeguard adopted in

precognition experiments of making a cut in the pack at a point determined by the choice of a number from a table of random numbers at a point determined by the fall of a number of dice would seem to be a sufficient guarantee that the number of right guesses in the absence of precognition will not be significantly different from mean chance expectation. In the same way, precognition cannot be a plausible explanation of success in a PK experiment if the target order and other experimental details are determined by some method (such as the selection of target order by Latin squares) which is not influenced by the free choice of the subject or the experimenter.

Every parapsychological experimenter is aware of the necessity for rigidity in the design of his experiments. There is some tendency to treat rigidity as the only problem of experimental design. A rigidly designed experiment is not of much value, however, if it is carried out under conditions which are such that paranormal success is unlikely to occur. We must, therefore, consider also the requirement of *fruitfulness*.

Certainly we do not yet know enough about the conditions necessary for fruitful parapsychological experimenting but we do know something. It appears that a favourable psychological atmosphere for parapsychological success is that of friendly informality. Experimenters may and should encourage this friendly atmosphere without relaxing any experimental precaution. Rigidity without fuss should be the rule for the experimental session. There are indications, too, that long sessions are to be avoided, and experimenting with subjects who are tired, ill, or hostile to the experimenter. Change of external situations (other than those demanded by the design of the experiment) also appear to be favourable to success. The use of subjects known to score well and the avoidance of experimenters who seem to inhibit phenomena are also valuable for fruitfulness, although for certain types of enquiry (such as discovering how ESP ability is distributed in a population) the method of selecting gifted subjects is ruled out by the nature of the enquiry.

The principle of *economy* is also sometimes overlooked in the designing of an experiment. Our time is always limited and our aim should always be to get the maximum information in the time available. It is, for example, an offence against economy to make many more observations than is necessary to give sufficient grounds for confident assertion as to the matter under investigation. Let us suppose, for example, that we are trying to find out whether a subject can make a better ESP score in the dark than in the light, and that we carry out 80,000 guesses under both

conditions and find a difference with significance indicated by $P=10^{-20}$. This not very important conclusion would have been established with a very much smaller number of experiments, and the last 75,000 guesses have been a waste of time which could have been more profitably spent in doing another experiment: for example, one designed to find out how widespread in the population was this superiority of the one condition.

It is true that Dr Soal and Mrs Goldney carried their experiment with Shackleton to a point at which P was 10^{-35} , but this experiment served a special purpose in parapsychology. It is part of the evidence that psi processes take place, and the high level of significance rules out the possibility that this is a selected best result from a number of unsuccessful experiments. The experiment should not, in this respect, be taken as a model for other experiments which have a different and more limited aim.

Defective design may also cause loss of time in other ways than by unnecessary experimental length. For example, PK experiments used often to be done with unequal numbers of throws for different target faces. These can be properly evaluated, but with considerable loss of information as compared with an experiment of the same length with all target faces equally frequent, and at the cost of much heavier statistical work. In considering economy, it is necessary to bear in mind that time spent on statistical work is also part of the time spent on the experiment and in a badly designed experiment it may be a large part. An experiment carried out with a complex and not carefully thought out design may at the end present a very difficult statistical problem. It is better to spend time before starting the experiment in planning a design as simple as possible which can also be simply assessed statistically.

A badly designed experiment may also give less than the full information that could be extracted from it. For example, a PK experiment may be done to test for terminal salience in which the targets are always presented in the same order. After the experiment is completed, it may be realized that any effect observed could be the result either of a tendency to score at certain positions of the series or to score on certain targets. If the target orders had been suitably randomised and the results treated by the analysis of variance, both questions could have been answered from the same series of results. It is generally an economy to design an experiment so that as many questions as possible can be answered from the one series of results.

There are in particular two faults of design on which I should like to comment since they are still found in parapsychological work, although less commonly than they used to be. Many

experimenters are still unable to understand that they are faults. These are : (1) failure to mix up experiments of different types between which it is intended that a comparison should be made, (2) failure to deal appropriately with cases in which each subject and not each guess must be treated as the unit for answering the question which the experiment is intended to answer.

The mixing up of experiments of different types when a comparison is to be made has long been a standard procedure in experimental psychology ; it is surprising to find it still sometimes neglected in parapsychology. Let us suppose, for example, that on Monday, Tuesday, and Wednesday, an experimental subject is successful in an ESP experiment and on Thursday a possible normal explanation of the success occurs to the experimenter. So on Thursday he introduces a new precaution X and finds that the subject scores only as expected by chance, and concludes that the precaution X prevents ESP success.

This is quite all right as an exploratory experiment, but a wholly insufficient ground for asserting that X has any effect on ESP success. We know that ESP success generally declines in the course of an experimental series and the successive introduction of new precautions may lead us to suppose that the last precaution has led to the disappearances of successes when, in fact, they would have disappeared in any case at that stage of the series. The objection to making such haphazard introduction of new conditions the basis for a conclusion as to a real effect of the condition X does not, however, depend on chronological decline alone. We know that subjects show day-to-day variation in ESP scoring. It may be that the new precaution made the subject score badly ; it may be that he had a bad day on Thursday and would not have succeeded then whatever conditions were used.

Although such an observation is improperly used as a basis for drawing a conclusion it should, of course, be noted and reported. Its value lies in the fact that it suggests a problem to be solved by a future experiment in which runs with the condition X and the condition not-X will be mixed up together. If, in such a series of mixed experiments, there is still found a significant difference between results under these two conditions, then, and only then, can it be safely concluded that the precaution X affects the rate of scoring.

The necessity for such mixing up was very commonly neglected in early parapsychological experiments, and these experiments often need re-evaluation in the light of this neglect. This applies both to experiments which are claimed to have established differences resulting from variation of conditions, and also those which

have been claimed to disprove the ESP capacities of a particular subject because the introduction of some precaution against a possible sensory cue seemed to lead to disappearance of ESP success.

The process of mixing up different experimental conditions is often referred to as 'randomisation'. In some cases the mixing up is done by the use of a table of random numbers and the two conditions may be presented in some such way as the following :
+ + - - - - + - + ... On the other hand, for many purposes it is equally satisfactory to use a systematic method of mixing up and to present the different conditions in such an order as : + - - + + - - + + ... If more than two conditions are being compared, it will be convenient to use some such method of mixing up as the use of a Latin square. In view of the variety of methods available, it is probably better to avoid the word 'randomisation' as a general term for this process and to use such a term as 'mixing-up'.

However it is done, some process of mixing up should be regarded as an essential feature of the design of any experiment intended to investigate differential effects in parapsychology. Haphazard variation of conditions should be found only in the exploratory stage of experimenting.

The second error in design which was mentioned above is that of failing to deal appropriately with the results of experiments in which each subject (or sometimes each experimental occasion) must be treated as the unit for answering the question the experiment is designed to answer. This can best be illustrated by an example. Let us suppose that the question to be answered is whether blind people show a different degree of ESP capacity from seeing people. The experimenter has, let us say, taken a group of 6 blind persons and 6 seeing persons (both groups having been selected by some appropriate sampling method) and has given to each member of both groups 16 runs through an ESP pack of cards. The mean chance expectation for each member will then be 80 hits. Let us suppose that the following scores have been obtained :

Blind group	82	87	104	108	88	78	Total	547	Devn. from expn.	+ 67
Seeing group	73	72	84	76	84	71	Total	460	Devn. from expn.	- 20

Many researchers are content to make the comparison in the following way. The difference between the scores of the two groups is 87, the standard error of this difference is 27·71, the critical ratio is over 3, and the likelihood of this difference occurring in a random sample of scores is about 1 in 1,000, so the difference is significant and the blind are proved to have better ESP powers than seeing people.

This is, however, an erroneous conclusion based on a wrong method of estimation for the question asked. The two samples are indeed proved different in ESP ability, but this may be due to a chance difference in the samples due to two high-scoring subjects happening to have been included in the 'blind' sample whereas none happen to have been included in the 'seeing' sample. The difference is consistent with both samples having been drawn from populations with the same ESP ability.

In order to determine whether the difference between the samples indicated a real difference between the populations from which they were drawn, it would be necessary to use the method of the contingency table. One could, for example, make a four-fold table showing the number of individuals scoring above and below mean chance expectation in both groups, as below.

	<i>Above</i>	<i>Below</i>	<i>Total</i>
Blind	5	1	6
Seeing	2	4	6
	<hr/>	<hr/>	
Total	7	5	

The deviation from expectation in each cell is only $1\frac{1}{2}$, and the probability of so great a deviation (in either direction) occurring by the chances of sampling is (by the exact method) .25 which is clearly insignificant.

It is true that in this case I have assumed a smaller number of cases than is likely to be used by an experimenter, but merely increasing the number of cases does not get over the necessity for using the method of estimation by a contingency table. A mere estimation of the significance of the difference between the totals of the two groups as evidence for a difference between the populations from which they were drawn (which is here the question at issue) is altogether misleading since it ignores the possible error due to sampling. Increasing the number of tests given to each subject will, if the correct method of estimation is used, have the advantage of reducing the relative size of the error variance as compared with the variance due to the cause under investigation. It will, however, also increase the amount of over-estimation of significance if the wrong method of estimation is used.

Difficulties of the same order may arise even in cases where a comparison is made between scores of the same set of subjects under different experimental conditions. Let us suppose, for example, that the top line in the above table refers to the ESP scores of six subjects under hypnosis and the second line to the scores of the same six subjects in the normal condition. Let us suppose that each entry refers to sixteen runs all done on the same occasion. The score differences in favour of the hypnotic state

are: +9, +15, +20, +32, +4, +7, with a total difference in favour of the hypnotic state of +87.

It would still be an error to estimate the evidence provided by this experiment that the hypnotic state produces a difference in scoring rate by dividing this total difference by a theoretical standard error of 27.71 and so getting a P of .001. Such a method of treatment does not take account of the possibility that the same subject may score at different rates on different occasions as a result of causes other than that of whether he is or is not in the hypnotic state. We have six pairs of scores obtained on different occasions, and we must ask what is the likelihood by the chances of sampling that we should have got so much a different sampling of scores on the hypnotic occasions than on the normal occasions on the null hypothesis that the hypnotic state does not itself affect the score.

To answer this question we must use some method that treats each occasion (or each difference between two occasions for the same subject) as a unit. Two methods are available. We may ask what is the likelihood by chance alone that we should have obtained the observed result that every hypnotic score differs in the same direction from the normal score. The answer is obviously $P = 2/2^6 = .03$, which is just below the maximum value commonly accepted for significance. Alternatively we may calculate t with 5 degrees of freedom from the six differences, and we get about the same result: $t = 3.44$, $P = .02$. In both cases, the significance is far below the value that would have been attributed to it if the experimenter had used the critical ratio of the total difference.

What gives this experiment its special character which makes the more familiar methods of assessing significance inappropriate is the fact that the two conditions of experimenting cannot be mixed up within the same occasion. Another case in which a comparison is made between two sets of scores of the same subject to which the same considerations apply is that in which each subject is tested with and without the use of a drug. One of the above methods of assessment must then be used. When, on the other hand, the two conditions can be mixed up within the same occasion, the more ordinary method of calculating a critical ratio from the difference between the totals under the two conditions is, so far as I can see, quite correct.

Where the considerations urged here are applicable, they obviously bear not only on the method of estimating significance but also on the general design of the experiment. The experimenter must use groups of subjects large enough to detect any difference which may exist. He must not suppose that multi-

plying the number of tests given to each subject can make up for an insufficient number of subjects ; that is what he is liable to do if he does not realize the point which is here being discussed.

In pointing out some faults of design which are still found in the work of experimental parapsychologists, it is not my intention to suggest that there is anything basically wrong in current parapsychological experimentation. In some respects, the best parapsychological experimenters are more sensitive to some of the dangers of drawing false conclusions than experimental workers in other fields. On the other hand, there are more people doing parapsychological experiments without a basic training in experimental method, and it is a field in which there are so few workers that we cannot afford to spend time in experiments which are less convincing than they might have been if problems of design had been more carefully thought out.

REVIEWS

THE EXPLORATION OF ESP AND HUMAN PERSONALITY. By J. Fraser Nicol and Betty M. Humphrey. (*Journal of the American Society for Psychical Research*, Vol. 47, No. 4, October 1953).

This is perhaps the most ambitious of the attempts hitherto made to discover correlations between levels of ESP scoring and personality factors in the mental make-up of the guessers. The earlier studies used, for the most part, projective or questionnaire methods by means of which a group of guessers was, with respect to some single personality characteristic, divided into a section A deemed to possess the characteristic, and a section B which did not possess it. The average score for the whole group on some ESP test was then evaluated, and the experiment was considered successful if there was a significantly greater percentage of persons in section A who scored above the average level than in section B. The present experiment, however, goes beyond this simple dichotomy inasmuch as a considerable number of different factors were correlated to ESP scoring and regression lines worked out to show how the scoring rate varied with the amount of the personality characteristic under consideration.

Three personality questionnaires, in addition to an 'annoyance test', were employed by the authors—that of Guildford, of Guildford and Martin, and one by Cattell which is still undergoing standardization. The first of these three is concerned mainly with extravert-introvert qualities, the second contains scores for five measures of temperament, while the third tests no fewer than

sixteen factors of personality. In all, nineteen characteristics were selected from the three questionnaires and the scores obtained were correlated with the ESP results.

The subjects of such questionnaires are required to answer Yes or No to a series of simple questions related to everyday situations and the value of any investigation based upon them depends essentially on the candour and sincerity of the persons interrogated. Many people, it must be admitted, will be loth to confess even to themselves that they are lacking in certain desirable social qualities, and one could scarcely imagine George Borrow, say, answering many of the questions truthfully. There is therefore an element of uncertainty connected with such tests.

In the present experiment clairvoyance tests were given individually to thirty-six young people of whom twelve were men, and each percipient was made to run through sixteen packs of standard ESP cards which had been previously ordered by means of Kendall and Smith's table of random numbers. Different packs were used for every run. One of the experimenters handled the cards behind a screen which had a small hole in its centre, while the other recorded the guesser's calls. Two separate conditions, referred to as the 'unknown' and the 'known' types of test, were alternated in runs of twenty-five cards. For the 'unknown' condition, the guesser was not told his score until the end of the run, while in the 'known' type of experiment the experimenter showed the target card that had just been guessed at the little window so that the guesser was informed immediately of his success or failure. During eight of the sixteen runs for each subject, Dr Humphrey handled the cards, while in the remaining eight Mr Nicol performed this task.

Significant correlations either at the five per cent or at the one per cent level were established between ESP scoring and each of eight different factors of personality. Coefficients of correlation were worked out separately for (a) scores under 'known' conditions, (b) scores under 'unknown' conditions, and (c) total scores.

By far the most striking correlation was that obtained with the factor of self-confidence. For 'total scores' this gave $r = +.55$ with a probability of $P = .0015$. In fact, out of the 30 persons tested for this quality no fewer than 81 per cent of the 16 self-confident guessers obtained scores that were above the average of the whole group, while 79 per cent of the 14 'un-confident' subjects made scores that were below the average score.

The next highest positive correlation was with the factor of emotional stability. This gave $r = +.47$ for total scores, with $P = .01$, and $r = +.56$ for the 'unknown' scores, with $P = .0015$.

Other factors showing significant positive correlations with ESP scoring were *Freedom from Nervousness* and *Freedom from Depression* while *Nervousness* and *Worrying Suspiciousness* were negatively correlated to ESP. The first two factors, however, were themselves very significantly correlated with self-confidence.

As regards the quality of self-confidence, it will be recalled that this was a predominant quality displayed by our own subject Basil Shackleton who was confident not only of his ability to guess the figures on cards but also in his belief that he was able to perform other feats which most people would consider very extraordinary indeed, or even impossible. On the other hand, it is puzzling and somewhat depressing to recall that David Kahn and Ulrich Neisser could find no significant correlations between ESP success and self-confidence in their group experiment. It is equally disturbing that Donald West was unable to repeat the findings of Dr Humphrey reported in her work with 'expansive' and 'compressive' subjects. Such contradictions make one feel, in pessimistic moments, that ESP functioning exhibits no law or order of any kind whatsoever! We must also note that on the grand total of 14,400 cards guessed by all the subjects there was a positive deviation of only 29 from the chance expectation, which is less even than one standard deviation (48).

From the regression line of self-confidence on ESP score, the authors were able to construct a simple formula which should answer a question such as the following: Given that a subject's confidence score is 3, what will be his expected ESP score? The formula gave fair agreement with certain of the scores actually observed, but other scores were far off the regression line and this suggested that there were other personality factors perhaps only slightly correlated to the self-confidence factor which were producing aberrations in the scores. The factor of 'emotional stability', for instance, was highly correlated with ESP but only moderately correlated ($r = .30$) with self-confidence. In order to obtain the measure of association between ESP and these two variable factors of personality, the method of multiple correlation was employed, which method, it is important to observe, automatically eliminates the effect of any correlation which exists between the variable factors themselves. The multiple correlation coefficient R was found to be .6538 and an analysis of variance procedure showed that the regression on ESP for the combined factors was a highly significant one with a probability of about only 1 in 1600.

Two additional regression coefficients were computed during the course of this work, and from these the authors were able to devise a more reliable formula for predicting ESP scores given

the scores for self-confidence and for emotional stability. How well or badly such formulae of prediction would work with fresh batches of subjects tested under similar conditions it is, as yet, impossible to guess. For the benefit of those workers who wish to repeat this interesting investigation, Nicol and Humphrey observe that it is useless for the experimenter to carry out only a small number of runs (say three or four at each session) since their own work showed no correlation that was significant on the first four runs and none very significant on even the first eight runs. Sixteen runs per subject at a sitting should be the minimum, and it is of interest to note that sixteen was the precise number for an evening's work with our own subject Mrs Stewart. Two other findings we have space to mention.

The 'unconfident' subjects started off apparently by scoring almost as well as the confident ones, but after the first run of a page of four runs their scores would tend to drop suddenly to below chance level and remain there for the rest of the page. The confident people, on the other hand, maintained their positive deviations to the end of the series.

Another very curious result was that for the series during which Mr Nicol sat behind the screen with the cards, the subjects, whether confident or unconfident, stable or unstable, nervous or not-nervous, made slightly higher scores for all six categories than in the series for which Dr Humphrey was behind the screen. Did Mr Nicol's air of businesslike determination put the guessers off their stroke, and were they pacified when the milder Dr Humphrey emerged into view?

To sum up, in spite of certain exceptions mentioned above, the general tendency of this investigation is to confirm the conclusions of many earlier studies that, in clairvoyance tests at any rate, the highest scorers are those who possess extravert qualities such as confidence, emotional stability, freedom from 'nerves' and depression, calmness of disposition etc.

The reader of this very careful report will doubtless ask many questions. If, for instance a further series of experiments had been carried out with the self-confident, emotionally stable group, would they have continued to score at a level a little above chance expectation? Possibly not. But though the practical value of these findings as an aid to the discovery of high-scoring subjects may not at present be very great, they provide strong reasons for the belief that there is far more in ESP than statistical artifact; that, on the contrary, it is correlated with certain human qualities in the guessers themselves.

S. G. SOAL

GHOSTS AND POLTERGEISTS. By Herbert Thurston, S.J. Edited by J. H. Crehan, S.J. London, Burns Oates, 1953. ix, 210 pp. 16s.

Father Thurston, well known as a student of psychical phenomena, brought an accurate and scholarly mind to the investigation of poltergeists, ancient and modern. He firmly believed in their reality 'and in the impossibility of finding any natural explanation of their recorded activities'. In this volume his biographer, Father Crehan, has collected his papers on the subject contributed to various periodicals. Apart from a brief introductory essay and some even briefer conclusions, the material of these papers is wholly historical, and they not only supplement Mr Sitwell's *Poltergeists* (1940) with a wealth of further evidence, but tell their story with a minimum of imaginative writing, and almost without comment. Colour, of which there is ample, is provided by the facts themselves and by the original records here quoted. Probably there is little new to be said by way of explanation of these phenomena, but anybody who desires to build up a theory of them, on the lines of Tyrrell's work in *Apparitions*, would find this book a useful and reliable starting point.

L. W. GRENSTED

THE JOURNAL OF PARAPSYCHOLOGY. VOL. 17, No. 4, December 1953. Durham, N.C., Duke University Press. \$1.50.

The Editorial by Dr Rhine is on the pattern of history in parapsychology with a forward glance into the future.

Dr Cadoret reports an experiment on the effect of drugs on ESP. Amytal and dexadrine were used. The experiments were carefully designed and some effects were found but at a low level of significance.

An article entitled 'Science and ESP Research' is the concluding chapter of the new book *Modern Experiments in Telepathy* by Bateman and Soal. This chapter suggests that the whole book will be a valuable contribution to the subject.

'A test of the relationship between ESP and PK' by K. Osiris describes experiments designed to find out whether a combined ESP and PK task is performed as a two-step or a one-step process. The latter alternative (called by Foster the 'diametric hypothesis') seems to be indicated by Osiris's experiments, although the level of significance of the results is not high enough to establish this conclusion with much certainty.

An obituary notice of the Rev. Drayton Thomas is contributed by Mrs Salter.

R. H. THOULESS

CORRESPONDENCE

'SERIALISM AND THE UNCONSCIOUS'

SIR,—Mr G. F. Dalton's article 'Serialism and the Unconscious' in the January-February 1954 number of the *Journal* impresses me as being a fundamental contribution toward exploration of basic problems relative to (a) precognition, (b) parapsychology in general, and (c) the nature of human consciousness. If Mr Dalton's basic position is valid, we shall never really have more than a superficial understanding of psychical phenomena—or of the meaning of our own lives—until we assimilate and begin to make systematic use of these basic concepts. If Mr Dalton's essential contention is valid, these concepts, relating to the serial observers in their serial times, provide the only vestibule through which entry can be gained into the real edifice of our existence. But, if any such potentially momentous significance be granted, even tentatively, the theory presented by Mr Dalton must either be proven to be basically invalid, or be shown to be in need of modification and improvement, or be adopted—at least as a working hypothesis.

May I, therefore, raise certain difficulties which seem to me to be fundamental, but which appear to call, not for the abandonment of Dalton's version of serialism, but rather for a further modification of his revision of Dunne's theory.

The clue to this basic difficulty may be found in Dalton's version of the concept of *intervention*. He cites Dunne's theory as holding the following view :

Events which are in the future for O₁ are already present in the more fundamental 'now' of O₂. The only thing which can alter these events is an intervention by . . . 'a higher-order observer'. [p. 226].

Dalton expresses his own view in the following terms :

There are several spontaneous cases in the literature which strongly suggest that . . . the future can be changed by intervention [p. 230]. If intervention occurs at all, it must be possible for any observer except the first to intervene [p. 231]. Education consists in the creation of an artificial environment, to which the child must respond with certain alterations of behaviour, considered to be socially desirable. But only the child himself can actually effect these alterations ; and in so doing he must make an effort of will, i.e. an intervention [p. 233].

Does not this (for Dalton, crucial) concept of intervention involve the logical fallacy of the uncontrolled middle? To say that 'the future can be changed by intervention' implies the use of the term *future* in two incompatible meanings in the same syllogism, which may be spelled out as follows :

MAJOR PREMISE : The future (where future means that which is really going to happen) can be determined to some extent by purposeful decisions in the present.

MINOR PREMISE : O₂, on the basis of precognition as to the future (where *future* means what would have happened in the absence of intervention), can intervene purposefully in the T₁ present so as to substitute a different future (where *future* means what really is to happen).

CONCLUSION : The future (used ambiguously) can be changed by intervention.

What is going to happen is going to happen. The apparent 'change' is actually merely a difference between an anticipated or falsely precognized future and what actually turned out to be the true event. The conception of an 'observer 2' who sees the actual future, and then changes that actual future to something else, appears to be simply a confusion in logic. What actually might occur, in an illustrative instance, might be something like the following. Observer 1 is aware, on the basis of past experience, scientific knowledge, and the like, that certain present events are likely to lead to certain kinds of future events—as, for example, that two cars approaching the same intersection at equal distances and at equal speeds will collide, and that the course of his own car can be altered by turning the steering wheel, while its speed can be reduced by stepping on the brake. This common-sense perception of alternatives in Time 1 leads him prudently to slow down, or to change his direction, so as to avoid a collision. Now, Observer 2 (according to Dalton's hypothesis) is aware of the events as extended in three space-dimensions and also in the Time 1 dimension. But the Time 1 dimension has an infinite number of possible alternatives (what in science-fiction is called 'alternative Time-Lines'). Even Observer 1 is aware of these alternative potentialities—as, for example, that in order to avoid a collision he may either swerve to the left and possibly collide with another car coming from the opposite direction, swerve to the right and turn over in a ditch, or hold his course, jamming on the brakes and hoping that his car will stop in time to avoid the collision. If Observer 2 perceives the actual future, we may conceive of him as perceiving the future avoidance of a possible collision with a car invisible to Observer 1, due to a precognitive warning. The *future* perceived by O₂ would not be the collision (which failed to occur) but the total space-time configuration, of which the warning and the avoidance of the accident were integral parts.

If this is correct, certain individuals who become psychically

sensitive to the precognitive awareness of their own O2's would become wiser and more successful in life because of the fact that they patterned their decisions in Time 1 not merely on the basis of ordinary prudence, but also on the basis of precognitive knowledge. The difference between a non-psychic observer and a precognitively sensitive one would then be a good deal like the difference between a nearsighted driver of a car who has forgotten his glasses, and a driver with 20-20 vision. The nearsighted driver would (of course) be far more likely to have accidents than the man who could see clearly the speed and direction of approaching cars.

Now, if sleep and hypnosis involve the abeyance of the O1 consciousness, it might reasonably be supposed that death also means at least the full emerging of O2. Difficulties in mediumistic communication on the part of personalities who survive death might then be due to difficulty of re-establishing the O1 point of view on the part of the communicator. On the other hand, the experience of a sensitive person who is still physically embodied might be regarded as being amenable to insights acquired telepathically from surviving friends who are more fully in possession of the O2 consciousness.

The above difficulty is not merely verbal. The discussion of education in Dalton's article becomes irrelevant when the conception of *intervention*, based on his ambiguous conception of the future, is eliminated. Also his major discussion of repression, as based on that concept, must be completely reconsidered, if the term *future* is used unambiguously to refer to what actually is going to happen, and if wise planning—whether by common sense or with the aid of precognitive awareness—are recognized as involving, not intervention, but simply more or less clear-sighted exploration of alternative future possibilities. If Mr Dalton's hypothesis can be revised along these lines, it may prove to be one of the most stimulating and fruitful developments in parapsychological theory which has appeared in recent years.

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Sir,—I should like to offer some comments on Mr Dalton's most interesting paper 'Serialism and the Unconscious' in No. 678 of the *Journal*.

On p. 230 he mentions a woman's dream of being burnt alive in a hotel fire, representing it as a warning, with an opportunity to intervene and thus to prevent the fatal accident. In my opinion the dream observation merely points to her precognition—i.e. to

her being fully aware—of the fact that *she would not be at the hotel* at the time of its destruction. Her leaving the place is not to be attributed to a 'warning' and its implication of the possibility of individual intervention, but simply to her *absence at the time of the fire*. This may appear strange, not to say contrary to common sense and therefore nonsensical, but the principle of a non-causally inter-connected and integrated universe like ours¹ implies the possibility that previously leaving a place may somehow be interrelated with, and therefore in a manner of speaking 'due' to a later absence, in the same way as this later absence is quite obviously due to the previous leaving. If the dreamer had not been warned against the fire in which she was predestined *not* to be burnt alive, she would have left for some other, probably more plausible reason. Of course there is no direct evidence as yet of present events 'resulting' (as it were) from future ones. But has there ever been any evidence either of the possibility of personal intervention—of a voluntary individual act successfully dis-integrating even part of the 'attributeless existence'² of the universe?

Any sequence of events can be explained both ways ; Mr Dalton's and mine. My own reasoning is based on the axiom of absolute non-causal interdependence and attributeless existence of all space-time co-ordinates (i.e. of *numbers*)—a rather non-euclidian axiom, I am ready to confess.

From a determinist's point of view both past and future events are therefore so completely interrelated that any future event may 'cause', as it were, its previous observation in certain circumstances. So may such negative events as being absent at the time of some incident. Dreaming of a horrible death you have escaped from is no evidence at all of having been warned against it (warned by whom?) and of having been given a chance to prevent it. Nor does any ensuing act furnish such evidence : it may as well be regarded as being non-causally linked, or inextricably entangled with the future event observed by the dreamer—in this case, her absence at the time of the hotel fire. In my opinion the odds are entirely against any hypothetical intervention, because both Space (left or right, up or down) and Time (past or future) may *sub specie aeternitatis* be presumed to be completely fixed and stabilised by

¹ Cf. Dr J. M. J. Kooy, 'Space, Time, Consciousness and the Universe' (*Tijdschrift voor Parapsychologie*, 1954, pp. 127–68); Kooy, 'Helderziendheid in de Tijd' (*De Nieuwe Stem*, 1949, pp. 711–20); J. C. M. Kruisinga, 'Het Tijdsprobleem' (*De Nieuwe Stem*, 1949, pp. 703–10). The first paper by Kooy is in English throughout.

² Cf. Dr Victor van Vriesland, *Grondslag van Verstandhouding* (Amsterdam, 1950).

their 'point-event' co-ordinates; i.e. by basic *numbers* which no human being is able to shift or control.

The knife-dream described by Mr Dalton falls into the same category. 'Just imagine—I might have stepped on it if my dream had not warned me!' No: the subject was scheduled *not* to step on it, and that is exactly what she saw in her dream. She was not warned against injuring herself, but only observing the particular spot of the cosmic plan or status centred round the knife, which spot most distinctly did not include the incident of cutting off her toe—because this incident was bound never to occur at all.

The 'warning' character of dreams of this sort is merely a symptom of the well-known tendency of dreams in general towards dramatisation and rationalisation; the tendency to keep up with normal waking experience and reasoning—with common sense, when all is said and done. But common sense has as yet been no great help to us whenever we seriously consider the time problem and the irrational inversions of reasoning it necessitates.

Many people are almost constantly dreaming of horrible situations in which they might become involved (if there is any such alternative as a 'might be' or 'might have been'). I cannot help regarding so-called warning dreams as superficially plausible and therefore dramatically 'convincing' instances of a mathematically impossible phenomenon. The present qualitative and quantitative evidence for precognition implies, at least in my opinion, the necessity for a complete revision of all our explanations and apriorisms, however plausible and even convincing. And this means that I can only see the case for personal intervention as an example of wishful reasoning by the individual wanting to grasp at least some of the minor controls of the universe.

I am confident that Mr Dalton will take no offence at my remarks in a language I do not sufficiently master. To one much interested in precognition and by its visible traces in dreams, his paper has been most refreshing and instructive.

J. C. M. KRUISINGA
Vriezenveen, Holland.

SIR,—Serialism may or may not be proved by other evidence, but J. W. Dunne's theory of Serialism is deduced from an initial fallacy. He defines his terms in terms of themselves. This inevitably leads to serialism. If x is what is included by x , this necessarily means that there must be another x including x , and a third including x_2 , and so on to infinity. Once the initial mistake is made the mathematics is inevitable, but it has an *Alice in Wonderland* premiss.

To say that a self-conscious observer is one who is conscious of

himself observing is not a definition, but a tautology. It would be nearer the mark to define a self-conscious observer as one aware both of his experience and also of himself as integrating that diverse and often contradictory experience as a finite entity. Whether or not the self is right about his consciousness of himself is another matter. One's dispassionate observation of one's own emotions may be only an intellectual survey of a part of oneself, but both the intellect looking on and the emotions surveyed are parts of a unity in the balanced individual; it is not really that another self observes. Or if this should be considered naïve, then let us say the personality is a bundle of separate entities, tied into one, but the self consists in the band that holds them together. In any case there is no logic in the idea of a serial self, recurring to infinity, nor of a recurring serial time, nor of any other thing, recurring because it is defined in terms of itself.

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Mr Dalton writes: I must plead guilty to a loose use of the word 'future', but not to a fallacy in logic. Professor Hart's syllogism is not mine; and he complicates matters by bringing in the question of mere probabilities or potentialities, inferred in the ordinary way from past experience. These probabilities may in a sense be called future, but they have no real existence, and action taken to avert them is not intervention. An ideal observer, knowing all the relevant facts, would know that a person of a given degree of intelligence and acting from given motives would inevitably take such action. The whole chain of events is automatic, following a normal cause-and-effect sequence in Time 1.

Apart from intervention, everything which is to happen could in principle be known in the same way. This gives us what I should call the 'Time 1 future', and it is this 'future' which can be changed by intervention. By saying that it can be changed, I of course imply that it has some present existence. Mr Kruisinga's description of his four-dimensional world has the same implication, and in fact it corresponds in most respects to the 'Oz field' of Serialism. But his *sub specie aeternitatis* view-point simply begs the question, by ignoring all distinctions of past, present and future. His world is one in which everything is completely fixed. But the world we know is full of apparent movements, and no combination of purely static factors can produce even an *appearance* of movement.

No matter how fixed the future may be, it is still distinguishable from the past, and any description which does not indicate this is

inaccurate. But any description of the four-dimensional world which *does* indicate it is accurate only momentarily, for the division between 'past' and 'future' does not stay still. Events which are now 'future' are becoming 'past'. There is movement in the four-dimensional world—the movement of the 'now-line' or 'present moment'; therefore there is time (apart from the Time 1 which is represented as space), and the possibility of change.

The 'Time 1 future' which is accessible to precognition is that part of the four-dimensional world which lies ahead of the 'now-line'. It is not the real future, but a pseudo-future which is really present to the dreamer. (This point was made by Professor Broad in his criticism of Dunne's theory, and I do not think that Dunne would have disagreed.) The real future may turn out to be similar to the pseudo-future, or may be quite unlike it; but in either case it is essentially a different thing. All we can say of the real future is that it will happen; *not* that it is bound to happen, since it *is* at present nothing at all, and nothing can be said about it in the present tense. It cannot be precognized, nor can it be altered by intervention, since one cannot alter something which does not exist.

Professor Hart and Mr Kruisinga both explain apparent interventions (such as the hotel fire case) in the same way: that is, that the precognition is of the event as it actually happens, i.e. of the real future. This brings up the question of causation. The event (e.g. the dreamer's absence at the time of the fire) is caused by the precognition; but the precognition is caused by the event; then what is the original cause of both?

Mr Kruisinga meets this difficulty by throwing out the idea of causation altogether. He speaks of non-causal linkage, presumably because in his world time does not exist, and an apparent sequence of events may be read with equal plausibility in either direction. But since he has not really abolished time, he has not abolished causation either. A causal sequence can exist in Time 2. To take the case of the hotel fire, the real order of events (in Time 2) is as follows: (i) The existence of certain factors—defective apparatus, careless employees, or whatever it may be—determines that the fire will occur. Other factors, such as the character of the dreamer, determine that she will be involved in it. At the time of the dream, all this is a fixed event in the Time 1 future, and a present reality in Time 2. (ii) The dreamer precognizes the event as it then stands, including her own fate. (iii) She wakes with the memory of the dream. This new factor—the memory of the dream—has no causal ancestor in Time 1. It initiates a new chain of events in Time 1, leading to the cancellation of the booking and the dreamer's absence from the fire. This chain of events is

determined from the moment that the dreamer wakes. At that moment, therefore, the Time 1 future is changed, this change being instantaneous in Time 2.

Intervention, then, is not a logical impossibility. It is, of course, a long step from this to showing that it actually occurs. Dreams of the type mentioned may be allowed some weight as evidence; and some support may be drawn from the *absence* from the literature of two other types of dream. One of these may be called the 'fatal precognition' type. In this, the action taken as a result of the dream brings about the very event which it is intended to avert. This is of course a very common motif in folk-tales and myths, but there are very few cases which have any evidential value. If this type were well established, it would be fatal to the theory of Serialism as it now stands.

The second type is a long-term precognition of an event which depends on the complex interaction of a number of people. Let us take, for instance, Professor Hart's example of a collision between two cars. The fact that each driver is there at the precise instant of the collision is due to an enormous number of factors—the behaviour of other road users, events in his own household or his place of employment, and so on. If any one of these factors had been different by a hair's breadth, he would have been a few seconds too early or too late for his undesired rendezvous. If an event of this kind could be precognized say a year in advance, it would mean that in that time there had been no substantial intervention by any of the dozens of people who might have influenced the driver, or by any of the hundreds or thousands by whom, in their turn, they might have been influenced. An accumulation of well-evidenced cases of this type would suggest that intervention is either impossible or very rare. But again the actual cases are few or weak, or can be otherwise explained.

One minor point in Professor Hart's and Mr Kruisinga's letters seems to require a word of comment. They both speak of 'warnings', and the word may suggest or imply the view that precognitive dreams are 'sent' to the dreamer by some external agency. It may be advisable to say that I know of no evidence for this view. The dreaming mind appears to draw on 'future' material as casually and easily as it does on material from the past.

In reply to Dr Wilson, I may say that not only did Dunne not define an x as what is included in x , but he spent much of his time pointing out the absurdities resulting from this fallacy. The Serialist view is precisely the opposite: the 'self' is distinguished as a separate and subordinate entity. In ordinary speech the two are constantly confused: to take a case, if I say, 'I think I am

getting a cold', the 'I' which thinks is not the same as the 'I' which sneezes. In general, that which observes can never be the same as that which is observed.

This question, like the last, could be argued at length on a philosophical plane, but this would mean going over ground which has already been fully covered by Dunne (see, for instance, *The Serial Universe*, Chapters IV-VI). Furthermore, the method of arguing that a theory contains a logical fallacy, and that therefore the evidence which is cited in support of the theory need not be examined, appears to me to be itself fallacious. The evidence may be invalid, but it just as likely that the critic's reasoning is faulty. The conflict must be fought out in both fields.

'ANTOINE RICHARD'S GARDEN'

SIR,—A rather important point in Mr Lambert's demonstration of the good faith of Miss Moberly and Miss Jourdain is the fact that the *Memoirs of Maréchal de Croÿ* were still unprinted when the first account of their 'vision' was published—and, presumably that the two English ladies had no possible access to the manuscript. The edition came out in 1906 and is thus described in the *Catalogue général des livres imprimés de la Bibliothèque Nationale*, i.v. Croÿ (Emmanuel, maréchal duc de) : *Journal inédit du duc de Croÿ, 1718-1784, publié, d'après le manuscrit autographe conservé à la bibliothèque de l'Institut, avec introduction, notes et index, par le Vicomte de Grouchy et Paul Cottin.*—Paris, E. Flammarion, 1906. In-8°, portraits et fac-similés. (Tomes I et II). Anyone, even a Bollandist whose profession is textual (and other) criticism, might be deceived by the title of this edition ('*Journal inédit*') and suppose that this was its first publication.

However, very extensive portions or extracts had been given to the public earlier than 1906, the titles of which are as follows :

(1) *Vicomte de Grouchy. Extraits des Mémoires du prince Emmanuel de Croÿ-Solre : I. visite à J.-J. Rousseau ; II. derniers moments de Voltaire.*—Paris, H. Leclerc et P. Cornuau, 1894. In-8°, 17 p. (Extrait du *Bulletin du Bibliophile*.)

(2) *Mémoires militaires du maréchal duc de Croÿ-Solre (1745-1761), extraits choisis par M. le Vicomte de Grouchy . . .*—Paris, aux bureaux de la *Nouvelle Revue rétrospective*, s.d. In-12, 158 p. Publié dans la *Nouvelle Revue rétrospective*. Année 1894.

(3) *Journal d'un voyage en Angleterre en 1766 (par le duc de Croÿ), publié par le Vicomte de Grouchy.*—Paris, bureau de la *Revue britannique*, s.d. In-8°, 53 p. (*Revue britannique*, 71e

année, n° 3, mars 1895. Mémoires, mœurs.—Extrait des Mémoires du duc de Croÿ.)

(4) Mémoires du duc de Croÿ sur les cours de Louis XV et de Louis XVI, publiés par M. le Vicomte de Grouchy.—Paris, aux bureaux de la *Nouvelle Revue rétrospective*, 1897. In-12, 427 p. (Extraits de la *Nouvelle Revue rétrospective*. Années 1895-1896.—Extraits allant de la naissance du duc à l'année de sa mort.)

Did Mr Lambert try to ascertain if the relevant passages of the Memoirs had not been printed in the last years of the nineteenth century? Without throwing aspersions on the good faith of Miss Moberly and Miss Jourdain, or of either of them, it might seem that the publication in the *Revue britannique* or the very name of the editor, Grouchy (famous through the failure of general Grouchy to appear on the field of Waterloo), could have attracted even an English reader's attention on the Memoirs and on the autograph manuscript which contained the full text.

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Bollandiste

Brussels.

Mr Lambert's comments on the above are as follows :

I was not previously aware of the Grouchy extracts from the Croÿ Memoirs, and am grateful to Fr Grosjean for calling my attention to them. The comparison he suggests works out as follows. (I give first my references to entries in the Memoirs, followed by the dates of the entries, the subject, and the page number of the issue of this *Journal* (Vol. 37, No. 676) where I referred to the entry.)

(a) 1774, 29 April-10 May : Last illness of Louis XV (145).

(b) 1774, 29 August : Views on Chinese Gardens (142).

(c) 1780, 21 April : Visit to Trianon Garden (146).

(d) 1782, 8 June : Last visit to same garden (142 and 146).

The extracts cited at (1), (2) and (3) of the letter include none of the above.

The extracts at (4) include (a) and (c), but not (b) or (d). The entries at (a) are at pp. 180-214 of the volume of the *Nouvelle Revue rétrospective* for January to June 1896, and the entry at (c) is on pp. 348-9 of the same volume, which contains much else besides the extracts from the Croÿ Memoirs.

It was undoubtedly possible for the two ladies, or for either of them, to see these entries, either partially in print, or all four in manuscript, before 10 August 1901, but it seems to me extremely improbable that they did in fact see either source either before or after their 'Adventure'. It must, I fear, remain problematic

exactly how much or how little they had learned about the history of the Petit Trianon before August 1901. Their claim to have known very little indeed is, I think, supported by the unconscious testimony of their subsequent attempts to verify their experiences. Those attempts would surely have been more impressive if they had been preceded by any detailed study of sources, even though the details might have been forgotten, in the sense of being beyond normal power of recall.

One must admit, also, that the two ladies might have read more popular works, the authors of which had made use of unpublished sources, even if they had not seen the Grouchy extracts. The closing scenes of Louis XV's life, also, must surely have been recorded by others than the Duc de Croÿ. Yet even if, by some miracle of paranormal cognition, we could obtain complete lists of the sources which each observer had read, even casually, it would remain a mystery why, if cryptomnesia alone was at work, the two of them should have seen the same, or approximately the same, spectral objects.

'ESP PROJECTION'

SIR,—Arising out of the Conference on Parapsychology at Utrecht last summer, Professor Hornell Hart, Professor of Sociology at Duke University, is directing an international enquiry into 'ESP Projection', in other words into veridical, apparently out-of-the-body experiences.

Some of these apparent 'projections' are reported as having been seen by other people, at the spot where the 'traveller' believed himself, in his out-of-the-body state, to be, and this alone makes the subject of very great interest, particularly at a moment when G. N. M. Tyrrell's study of apparitions is again being widely read.

Professor Hart has asked me, among others, to try and collect for him cases of first-hand veridical apparently out-of-the-body experiences, which were reported to a third party before being independently corroborated. Facts and events reported by the 'traveller', which he could not by sensory means have known, are of course of particular value.

I should therefore be very grateful to any of our members who have had such experiences themselves, or who could obtain written *first-hand* reports and corroboration of such experiences from friends for whom they could vouch, if they would send them to me for forwarding to Professor Hart.

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